Clim. Past Discuss., 7, C899–C903, 2011
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7, C899–C903, 2011

Interactive Comment

Interactive comment on "Middle Jurassic-Early Cretaceous high-latitude sea-surface temperatures from the Southern Ocean" *by* H. C. Jenkyns et al.

Anonymous Referee #1

Received and published: 24 June 2011

The manuscript by Jenkyns et al. presents a new data set of TEX86 data from Jurassic to early Cretaceous high-latitude sites around Antarctica. These data show constantly warm SST that are interpreted in a way to rule out significant ice sheets and are used to support or reject biotic and/or geochemical evidence for short-term cooling events during this time interval. While the presentation of the data is good and having this kind of data available makes a large step towards understanding long-term climate evolution during greenhouse intervals, large parts of the discussion chapters are not supported by the dataset presented. Given all uncertainties and weaknesses of the manuscript in its present form (see comments below), I can only recommend rejection. But I am sure that the authors can re-adjust the focus of their study to get this nice data set published

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Interactive Discussion

Discussion Paper

elsewhere.

My major concerns are:

1) The data set presented is of really low resolution. For some 50 Ma at Site 511 there are less than 50 data points. For Site 693 only 11 samples were measured. Given this resolution of the data, there is no way to discuss short-term events like potential ephemeral ice sheets on Antarctica (probably lasting 100-200 kyr) and also cold snaps (sensu Mutterlose et al.) would be represented by one or two data points only. As already stated by the authors in the last paragraph of the introduction, this paper deals with the long-term evolution of paleotemperatures and NOT with short-term events. So it should concentrate on these long-term trends rather than discussing short-term events in detail.

2) Along the same line comes the problem with stratigraphy. For both records stratigraphic control is vaguely assigned. But as stated already in the methods chapter, these age determinations are far from being precise or clear. This adds another large problem when using the presented data sets to discuss short-term events.

3) The TEX86 proxy that is used here the first time for such old sediments is treated as being a proxy without any problems and numbers are taken as real without even discussing potential problems. The organic-geochemical TEX86 proxy appears to mostly reflect the annual mean temperature of the upper water column (e.g., Wuchter et al., 2006; Kim et al., 2008), but does not necessarily correlate with SST (e.g., Huguet et al., 2007). Furthermore, sediment trap data show different temperature dependencies with increasing water depth (Wuchter et al., 2006). A yet unresolved disadvantage of this proxy, however, is that its results can be biased through the presence of terrestrially derived, fluvially transported branched and isoprenoid tetraethers as they were recently discovered in a number of terrestrial settings (see discussion in Weijers et al., 2006). Consequently, the BIT index should be presented for these data as one possible way to show that they are not influenced by terrestrial organic material. What happens

if you use another calibration? All these problems have, at least, to be discussed in the manuscript and the authors should be much more careful about their data and the interpretation drawn from it.

Some comments on individual chapters:

Introduction:

- Isotope data from well-preserved foraminifera are stated as being problematic. Please explain this statement in more detail because this is important for your discussion afterwards when you use foraminiferal data to argue in favor of the TEX86 data.

- Using the extrapolation of Maastrichtian bulk oxygen isotope data back to the early Cretaceous in order to say something about SST is simply geophantasy (even if it is published in Nature)!

Stratigraphy:

- Given the large range of uncertainties in stratigraphy, the manuscript would benefit from their indication in the respective figures. Do not give a rough indication of stages, rather plot the different biostratigraphies against the data set to illustrate the large uncertainties and to allow readers to get a detailed look at it.

Marine SSTs:

- Did we speak about mean annual temperatures or summer temperatures (a large difference in high latitudes)?

- Both described long-term trends in SST are in the range of 1-2° and therefore close to the resolution of the method. Maybe it gets clearer if temperature axis in Fig 3 is cut by 32° to see the real variability.

- High altitudes at Antarctica are, of course, the only way to develop Mesozoic ice sheets (see e.g. discussions in Bornemann et al 2008, DeConto and Pollard 2003, Fitzgerald 2002).

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Thermocline reconstructions:

- All this discussion depends on the reliability of belemnite data for temperature reconstructions. (and TEX86) But this is a matter of large debate and the authors should at least discuss the problems and not only use the data to come to conclusions about absolute temperature differences across the thermocline (and reject former approaches). All the latter discussion of this chapter depends on the reliability of the chosen data sets and totally neglects living depth, vital effects, inter-species effects, preservation. . . ..

- Furthermore, how realistic is a 14° difference between SST and subthermocline water masses in a region that is close to deep-water sources?

- Also, there is not a good connection to the other ocean basins during the early Cretaceous (and the Jurassic). So what could be the effect of different water masses that are sourced locally? What influence has the paleogeography on potential subthermocline water masses?

Early Aptian OAE:

- I am confused by the arguments for the occurrence of OAE 1a in this section. The authors cite biostratigraphic and lithologic results of Bralower et al for this but, given the uncertainties in biostratigraphy, how reliable are these? Furthermore, there is absolutely no clear peak in TOC that could indicate the OAE interval. TOC is at the same level for the entire Cretaceous section.

- Can you really correlate your section to OAE 1a in terms of carbon isotopes? The increase in δ13Corg lacks the pronounced negative excursion of other sections and just the increase itself can be anywhere in the Aptian/Albian especially due to the fact that TOC drops out of the system significantly parallel to your positive excursion and that the restricted nature of the south Atlantic could force local effects on carbon isotopes.

- The entire discussion about cooling and CO2 drawdown during OAE 1a is not backed-up by the presented data. There is only one more negative point (SST) that is even

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uncertain in its stratigraphic range (how can you correlate that to the distinct, short-term events that are the backbone of the drawdown theory?)

Evidence for cold snaps:

- This entire chapter deals with correlations that cannot be done based on the low-resolution data set and the weak biostratigraphic control of the sections. Furthermore, most of the discussed signals consist of single points.

- At the end there is no reason why ice sheets should not have existed as short-term interludes (100 kyr scale) within a warm greenhouse climate. At least you have no handle on that using the presented low resolution data.

- And at the end there is still the question: Where does the sea level drops that exist come from. That clearly is beyond the focus of this paper but still it is the most important question if ice sheets are ruled out as trigger mechanism.

Interactive comment on Clim. Past Discuss., 7, 1339, 2011.